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Scientific Life

Times of Fraud

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A scientist's career is not a smooth arc but rather a series of targets and deadlines, some more necessary than others. Several forms of research misconduct are specific to and incentivized by such deadlines, making a fine-grained understanding of those professional ecologies a necessary step toward preventing misconduct.

'Publish or perish' still captures much of the predicament of research misconduct, except that the issue is not simply what or where to publish, but also when to do so. A scientist's career is not a smooth arc but rather a series of thresholds and deadlines, some more deadly than others.

For instance, there is a time in a scientist's early career, prior to securing a permanent job, when s/he might be more prone to misconduct due to additional professional pressures and/or insecurities. The response to these pressures can take different forms, including altering an article's byline. We have learned that 'junior researchers who co-author work with top scientists enjoy a persistent competitive advantage throughout the rest of their careers' [1], which may explain why we find young scientists, especially some struggling to stay in the game, falsely list well-known scientists as collaborators.[i] At the other end of the professional spectrum, some senior scientists in the run for prestigious prizes that demand an ongoing engagement in research may be inclined to accept or expect 'honorary authorship' from their junior colleagues, even when their contribution to those publications may not warrant coauthorship.

Yet another example comes from the market for so-called predatory journals. Condemned for allegedly deceiving users into believing that they are quality venues, these journals promise to publish articles in a few days/weeks, often by skipping peer review. The problems with such journals are well known, but it should be noted that the unreasonably fast production time, for which they are ridiculed by established academics, could in fact be part of their market appeal. They may be beacons of hope for authors in a pinch. The editorial and production time frame of respectable journals may become a fatal constraint for scientists who quickly need to add a publication by a hard deadline: a job application, contract renewal, tenure review, and so on. If the choice is between losing a career because of one missing publication or quickly publishing something in an unknown but still Anglophone (and thus 'international') journal that is rarely read, the latter may not look like the end of the world.

Similar deadline-specific pressures may be behind the inclusion of fraudulent evidence in grant applications, or even the plagiarism of grant proposals by peer reviewers. In fact, grants may be more unnegotiable than publications because the very conditions of possibility of research (funding labs, providing graduate students fellowships, and sometimes the principal investigators' own salaries) depend on them. A grant deadline is thus much more than a grant deadline, which helps to make the submission of a fundable application, by legitimate or illegitimate means, look like a necessity. Some of these examples outline a 'borrowed time' model: engaging in misconduct to see another day.

But there is also another model based on a different kind of borrowed time, featuring distinctly different time horizons and practices. The ubiquity of image manipulation and duplication of photographic evidence shows that, in the age of digital editing tools, fraud has become virtual, quickly and easily produced with little more than a computer running Photoshop. While we are not yet seeing the production of fraud on a truly industrial scale, its production has become distinctly more efficient: editing photographic evidence or reprinting it verbatim with a different caption is much faster than faking it from scratch (as done in the predigital age). If it were not for meeting demanding publication targets, why would we find the same coauthors reproduce the same fraudulent image, or parts of it, 15 times in 10 publications as if they 'only ever made a handful of western blots which were forced to stand in for all possible instances in their many publications'?[ii]

One striking feature of current digital image manipulation is that it is relatively easy to detect, even without forensic software [2]. The apparent goal, therefore, seems not to avoid getting caught, but rather to postpone detection for as long as possible, usually by publishing in low impact journals that few read. This is different from the risk associated with a fraudulent grant proposal, which is mostly short-term. If you are lucky enough not to get caught during the review process, you may be in the clear, enjoying your borrowed time. Instead, the ongoing visibility of many quickly photoshopped images creates a never-ending danger of detection. Furthermore, once one article is flagged for concern on PubPeer, people will quickly start looking at the images in your other publications. Because of the specific modality of fraud involved, the chance of walking away unscathed drops dramatically as soon as the first red flag goes up. Within the paradigm of fast and cheap digital manipulation, fraud is not a one-off occurrence but a business model you keep pursuing until you get caught. Paraphrasing Thomas Kuhn, we might call this model 'normal fraud'.

This is profoundly different from traditional fraud, which seems to depend on one's long-term career as the temporal horizon for one's decision to fake: I fake it maybe only once in the hope that I will continue to have a career in science. By contrast, the masters of Photoshop would be naïve if they did not realize that, based on their business model, their careers as academic scientists are likely to be only a phase in their professional life. This suggests that they probably make contingency plans all along: a move to industry, to a university in another country, or to a different career altogether. And the time that it usually takes for misconduct allegations to be voiced and pursued, for a retraction to be eventually issued, and for possible litigation to run its course, is probably enough for these people to pivot toward their plan B, or simply reach that sweet deadline called retirement.

The point is not to condone any of these practices but to acknowledge that: (i) publication pressures and stresses are not evenly distributed in time, but spike by orders of magnitude around certain deadlines, each carrying different implications for one's ability to continue a career in science; (ii) we have scant evidence of how deadlines inform publications strategies, how that varies with professional seniority, and whether that has changed over the last few decades; (iii) we expect graduate students to fulfill research ethics training requirements, but we do not teach them deadline management and long-term research planning, skills they need the most at the beginning of their careers; and (iv) we do not have knowledge of which targets and deadlines are conducive of science worth reading and which ones, instead, are likely to induce goal displacements and incentivize misconduct.

The history of science and technology indicates that the competition for priority has been central to both Western science and patent law due to the definition of 'scientific claim' and 'invention'. Being construed as information, we think of them as valuable only if new. But if the timestamping of priority is fundamental to how we have organized science and intellectual property, other temporal targets like recurrent deadlines for competitive grants do not seem to fall in the same category. For instance, the federal grant system typical of science in the United States is advantageous to the university, allowing it to outsource much of its research budget while collecting overheads on it. It may, however, be suboptimal for science in general, producing incentives for misconduct while also contributing to the choice of fundable but not necessarily innovative research [3]. As Johann Bollen has recently proposed, there are other ways to differentially distribute funding according to the assessment of the quality of one's peers without relying on grant proposals and reviews [4]. The point is not to try to dispatch the grant system but rather to review the rationale of the very many time-specific targets that the social system of science has kept accruing, often without an assessment of their hidden costs. That includes metrics of evaluation and the expectations (and gaming) they enable [5].

Together with a review of targets and competitive deadlines to assess how much they contribute and how much, instead, they may incentivize misconduct or produce uninformative proxies of quality, we also need to develop a better understanding of what 'publish or perish' really means. The methodologies are probably less crucial than the questions. Data gathered from broad-based questionnaires could complement fine-grained qualitative ethnographic evidence, and vice versa. What matters is being able to reach into the trenches, mapping the everyday choices and tensions that researchers face, and understanding how that predicament changes during the stages of one's career, as we are beginning to learn from recent science and technology studies [6]. Retraction-based studies have given us a remarkably better view of the distribution, typologies, and effects of fraud, but we now need to move our gaze upstream to understand its genealogies and how they can be prevented.

Resources

[i]https://retractionwatch.com/2020/02/04/former-grad-student-forges-his-supervisors-authorship-and-gets-smacked-down/

[ii]https://forbetterscience.com/2016/06/21/mario-saad-and-the-return-of-the-wandering-western-blot/

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